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ABSTRACT

Methods for assessing institutional capacity for research and development (R&D) are examined. The paucity of research data on the topic is noted. Some general conclusions are drawn: (1) There is no set formula or checklist to provide an accurate assessment of institutional R&D capacity. (2) False negatives are more serious than false positives, so the selection criteria should be lenient to the point of flexibility, consistently favoring applicants or existing centers when there is some chance of success. (3) Leadership is significant, and the selection of a young scientist (35 to 45) with a history of successful contributions and favorable relationships with other people will help ensure a center's creativity. (4) The consortium form of research organization should be rejected under rare circumstances. A research firm is necessary for a concerted attack on a narrow goal, as R&D should be defined. (5) The current economy is favorable to organized research, due to a tight labor market and the scarcity of research funds for projects. (Author/LBH)

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ASSESSING INSTITUTIONAL CAPACITY: SOME CONSIDERATIONS FROM THE CRAFTLORE OF ORGANIZED SOCIAL RESEARCH

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Introduction: Setting the Problem:

The task of this paper is to discuss how one might go about assessing institutional capacity for R&D. Obviously, the sense of the task rests heavily on the impression that such capacity varies from organization to organization and varies in some lawful ways. While one does not expect to attain perfection in prediction, a reasonable scheme for assessing institutional capacity should be able to account for enough of the variation from place to place to make it worthwhile to use the scheme in allocating scarce resources so as to maximize returns. In the best of all possible worlds, such a scheme would also provide guidelines to organizations that wish to increase their capacities for R&D.

It should be emphasized at the outset that research on this topic is slight to non-existent. Although social scientists have computed the productivity of academic departments in terms of books and professional papers and even aggregated references in citation indexes, there has been little done on what it is that makes one department more productive in these senses than another. R&D in the social sciences is so recent an activity in an organized sense that no attention has been paid to even laying out a gross accounting scheme. There being little or no firm evidence to go on, this paper is more of a prolegemenon to research than a synthesis of existing empirical knowledge. As such it is highly dependent on the experiences and viewpoints of the author, and one can anticiapte that there are possibly large biases in selectivity of experience and of viewpoints.

At the outset it is necessary to recognize that a useful assessment scheme is useful only in the context of the problem of making choices

about action alternatives. In this connection one of the first assessments a decision maker has to make is to assess the relative dangers involved in Type I and Type II errors. If false positives are the greater danger -- i.e., funding organizations that should not be funded -- then one would want an instrument that was very good in weeding out potentially unproductive organizations, absorbing the risk of weeding out some potentially ones as well. On the other hand, if it is false negatives that present the greater danger -- i.e., rejecting organizations that were potentially productive -- then one would want to use criteria that were more lenient. Given the present state of affairs in education, it would seem to the present writer that the greatest danger would be to turn down an organization that had potential. This argues for a strategy of leniency in criteria, of accepting some dubious choices, especially when the measures of the criteria of choice are of borderline acceptability or contradictory. As the Rold Campbell Committee suggests, it if is not at all clear in which direction one should go in R&D efforts, then it is difficult to set up criteria of judgment that are precise enough to sort out consistently the good bets from the poor ones.

These considerations raise the question of what would a good R&D effort look like? How long a time range should R&D efforts take? At the broadest level, one would want to finance efforts that will be able to produce educational innovations that will further the goals set by NIE, are workable within a broad range of school systems, and will be

accepted by a broad range of school systems. To calibrate the time perspective involved, one might consider how long it took for the basic science high school curricular materials developed by NSF to be adopted by a wide range of school systems. It is appropriate to take such materials as a standard since it probably represents a lower bound on the time perspective, curricular materials being probably easier to disseminate than, say, changes in class room practices of teachers. It took at least a decade to develop the materials, find a distribution system, and to diffuse to a large portion of the educacional establishment. If we take a decade as a minimum standard then we need to think of an R&D effort that is at least a decade long with possibly another decade for dissemination. If this time perspective appears too long range, it should be borne in mind that judgment about the potentials of an R&D effort could probably be made long before the first decade has been finished. Hence this is not an argument for long range, double decade uncritical support but an argument that a relatively long perspective, possibly as much as five years, ought to be used as a time span for allowing an R&D effort to show its full potential.

In the last few pages we have presented arguments that amount to a proposal for leniency in the judgement of institutional capability. The arguments may be recapitulated as follows: first, the costs of false negatives are greater than the costs of false positives. Secondly, the time perspective on R&D is probably about a decade, an argument which suggests that it would be difficult to detect the success of an R&D

effort early in the performance of a unit. Thirdly, the goals of R&D should be divorced from the task of dissemination, especially since dissemination is a process that is likely to take as long as R&D itself.

Recipes for Failure:

One way to produce failures is to select incompetent persons or organizations to perform a task. However, an equally effective method is to so structure the task that it is impossible to complete, in what might be called the Augean stable method of producing failures. I have seen sufficient examples of the operation of this method -- usually unwittingly -- in the area of evaluation research that it is worthwhile devoting some space to it in this paper in order to draw attention to some of its features.

Perhaps the easiest way to insure failure is attract incompetence. A great proportion of the RFPs for evaluation researches present a task that is simply not possible to be accomplished within the time, scope, or funds advertised. An RFP which calls for the evaluation of a large scale program within the space of six months at an amount that would provide only personnel support is bound to be attractive only to those who are extremely hungry and/or foolish enough to believe the task can be done within the time period specified. Experienced investigators or research organizations would most likely refrain from submitting a proposal. Nor is this phenomenon restricted to RFPs for evaluation researches: the Performance Contracting Experiments were so set up that failure was unwittingly maximized. Insufficient setup time was provided for both the operating contractors and the evaluating

organization. Contractors had little experience and no provision was made for them to develop the experience: R&D had to be accomplished at the same time that they were being evaluated as if they had a finished product. And so on.

A second efficient way of producing failures is to make continually accelerating demands on performance. For example, if one initially sets a time limit on the production of a task at x months and then half way through re-sets the time limit at .75x and so on, then it is clear that it will be impossible to succeed in fulfilling the task. A similar tactic is to change tasks continually. The net effect of these management modes is to continually keep the performer off balance.

A third efficient method often related to the ones just described, is to change management frequently. To a new management everything done by a predecessor is suspect since it is mainly by clearly differentiating regimes that demonstrations of clear superiority of successor over predecessor can be made. The micro-history of R&D centers, as Roald Campbell's report indicates, had entirely too many examples of task shifts associated mainly with management changes.

A final mode of producing failaures is to group together tasks that are inherently inherently incompatible. The combination of research, development and dissemination is probably a set of antagonistic elements. They are antagonistic not in the sense of one task offsetting the gains of another but in the sense that the skills involved in one task may never be found in combination with skills necessary for another, or in the sense that the management of one of the

tasks is quite different from the management of another. For example, as a recent editorial SCIENCE suggests, political decision making is concerned with winning while the activity of science is concerned with knowing, a difference in orientation which makes it difficult for scientists and politicians to communicate easily. Similarly, it may be that the skills that are involved in research are different and perhaps antagonistic to those involved in the development of engineering applications and even more divergent from those involved in effecting the changes in social systems necessary for adoption of the engineered technique.

I suggest this possible incompatability of tasks not so much because I believe them to be so in the case of R&D in education, but because of the real possibility that such may be the case. It may even be the case, as some analysts of the failures of "hard" science R&D organization upon moving into social system applications suggest,* that R&D applied to social systems are more incompatible than in the case of "hard" science engineering applications. If we assume for the moment that such an incompatability holds in the case of R&D in education, then the task of assessing institutional capability becomes especially difficult if not impossible since under such conditions the likely outcome, whoever

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*Obviously, there are many other elements possible at work in such cases, as, for example, the fact that simple lack of knowledge concerning social systems cannot be made up for by postulating general systems models as applicable to social systems without adding considerable knowledge of the specific parameters in operation.

may be selected, is failure.*

The considerations set forth in this section were designed to remind the reader that the process of assessing institutional capability is not the only process that is at work in producing organizational (or individual) success or failure. There are structural constraints that can raise the chances of failure however good the selection process may be. In addition, structural constraints may lower the choices available (as, for example, by setting tasks that are deeply unattractive to talented organizations and individuals) so that only the incompetent and foolish compete.

The main import of this section is to question whether the nature of the taks set forth for R&D centers has been thought through. What would good R&D look like in a particular case? Is it possible to define a problem area -- e.g., reduction of disciplinary problems in junior and senior high schools -- in such a fashion that the issues are clear guides to the research efforts that ought to be carried out? Furthermore, if we are thinking of applied research, such efforts have to be focussed on problems defined in terms of policy related variables: it makes little policy relevant sense to define research on disciplinary problems as inquiry into behavior genetics since there is little policy relevance to such an inquiry. Assuming that research has been successful in a particular area, then the development of a working corrective model that can

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*One may also consider fostering internal specialization within potential R&D organizations such that there are separately organized and managed departments of R&D organizations in order to recognize necessary specialization. Or, one may decide to separate the tasks by creating separate organizations each devoted to a single task. Either adaptational modes has the same effect of not imposing on the individuals and organizations the necessity to carry out tasks that are in some sense antagonistic. I believe that this is the reason that most social research organizations separate out data collection departments from analysis departments.

be employed within the context of existing institutions needs to be undertaken. Pursuing the analogy to engineering, such development should be sensitive to the existing range of school systems, schools and teachers as well as pupils. The advantage of a "technological fix" (as Alvin Weinberg has phrased it) is that it is less operator dependent than a "social fix" and hence more easily adopted. But it seems less and less likely that major "technological fixes" are going to appear suddenly on the scene: a more likely development is a sensitive adjustment to the fact that the educational establishment changes by mighty small increments.

The Meteorological Model of Assessment of Institutional Capacity:

In the absence of any other information, the best prediction one can make of the future is that it will not be different from the present. The correlation of one day's weather with that of a succeeding day's weather is sufficiently high that it is a good bet to predict that tomorrow will be no different from today. Indeed, the efficiency of weather prediction efforts can be judged on how much better one can do compared to using the persistence meterological prediction. Similarly with humans and social organizations: it is clear that the best prediction one can make is that a productive individual will continue to be productive and that a highly productive organization will persist in its productivity, assuming that one has no information other than past productivity.

If we supplement the meteorological persistence model with a statement that humans and organizations that have done well in one field are

likely to do well in another related field of inquiry or application, then we have a decision rule for assessment of capacity that is perhaps the best one can do as an initial start. The question then becomes whether by the application of considerations other than past track record can one appreciably increase the accuracy of assessment?

There are two types of errors involved in the meterological model of assessment: first, there are organizations that do not have a track record because they are proposed rather than actual organizations, or there are organizations composed of persons who do not have established track records. Secondly, there are organizations and individuals who, for one reason or another, have track records that are inflated as predictors of future performance. A first start on improvements of assessment over the persistence model is to reduce these two sources of error.

To reduce the first type of error -- incorrect assessment in the absence of a track record -- it is useful to think of the following tactics: first, a proposed organization can be assessed, at least in part, by examining the track records of proposed principal investigators. I would be profoundly distrustful of a proposed organization that is to be composed of middle aged principal investigators who do not have an established track record. While a successful research organization is likely to have a positive effect on some of the people who join, it seems highly unlikely that middle aged mediocrity will be a stimulating environment to middle aged mediocrities. As a corrollary one should be more receptive to organizations composed of very young persons who have not

had opportunities to establish track records,* bearing in mind, of course, that there are other reasons against betting on the young in institution building (as we shall discuss later on).

The second type of error, betting on some organization or individual that is on the verge of decline is a little more difficult to detect. Organizations as well as individuals can become so devoted to their pasts that they are unable to move into the future without replicating the past. Furthermore, the past is most attractive when it has been marked by success. For these reasons, I would not be as enthusiastic about a successful institution or researcher that promises mainly to do what it or he has done in the past and look with more favor on one who has new ideas or proposes to enter an entirely new field.

The persistence method of assessment is not a first priority assessment tactic, as all would recognize. Even when all the signs are in the right direction, the persistence hypothesis depends very heavily on the size of the correlation between performances in adjacent time periods, a correlation which in the best of possible worlds is not high, possibly more on the order of .5 than .8, corresponding to about 25% of the variance in performance in any one period. Hence major efforts in improving assessment should be directed at moving away from the persistence model and efforts to fine tune the model to ones which promise to model the

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*It is difficult to establish precisely what is "young" and what is middleaged." I suspect that youthful innocence of productive activity is lost quite quickly after the Ph.D. for those who are going to be productive in the social sciences, perhaps in the first two years. Hence a person who is four years beyond the Ph.D. and has yet to show at least a few starts down a track record is not likely to move briskly when he is eight years beyond the Ph.D.

institutional practices themselves. It is to these sorts of considerations that we turn in the next few sections of this paper.

Charisma versus Organizations and Related Mysteries:

There are few concepts that cause sociologists as much trouble as that of "charisma", those special and unknowable qualities of individuals that characterize our great leaders and important innovators. To a sociologist individual qualities that are more clearly related to social origins and social role are more congenial: Indeed, it is likely that it is the revered ancestral position of Max Weber that keeps charisma strongly ensconced in the sociological thesaurus.

Yet there is so much experience of a personal and historical sort with charismatic figures, that it is impossible to do without the concept. Obviously, the concept has importance in the world of scholarly activity as well. There are "movements" in every field that are traceable to the influence of some single individual or institution. There are schools of social science centering around some founder and there are styles of social science that are traceable to the prominence in some period of one or another institution. The future promises to be no exception to the past in this respect: we can expect that charismatic individuals and leading institutions will play critical roles in at least some of the important changes that will take place in education (as well as other areas of life).

The questions which charisma raises for the task before this paper are as follows:

> .....How important are charismatic individuals in the research enterprise and within research organizations?

.....Is charisma a quality of individuals per se or is it a quality which emerges as a consequence of the mix of individual qualities and social organization?

.....Are there recognizable types of charisma that ought to be taken into account in assessing its importance in research and development?

It will be useful to start with the last question at the outset. The essential characteristic of a charismatic individual is that he or she appears to be endowed with some special widely recognizable "grace" that inspires admiration, awe, and is the grounds for exercising leadership. A charismatic individual may not necessarily be an intellectual innovator: indeed, we have many examples of persons whose charismatic qualities have been worked out in the furtherance of someone else's ideas.

It is possible to discern several types of charismatic leadership. For example, there are those whose charism: works best at a distance, whose inspiration is transmitted mainly through speeches, publications, and lectures but who seem to have an inability to work with others at close quarters. On the research side of intellectual life, there are the solo researchers and scholars who influence persons at other institutions but who cannot (or will not) coordinate the work of a laboratory group or research team. The social sciences may have more of this "inspirational" charismatic type than other fields because it is still possible to conduct solo research and scholarship in our field. Needless to say, an inspirational charismatic may spell disaster for an institution, especially when given a leadership role.*

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*This prognosis, moreover, has not stopped many such individuals from assuming leadership roles.

Another charismatic type of more interest here are those who work best with disciples, persons who are allowed close to such a leader in return for loyalty to his doctrines and even to his person. In such cases the charismatic leader and his disciples can be (and often are) highly productive. An institution built around such a leader, however, is a fairly fragile enterprise whose major strength lies in the intellectual potential of the charismatic figure. There are many examples of academic departments, research centers, and other institutions who have experienced abrupt declines on the decline or death of the leader. Disciplines without their leader are often pathetic ritualists, performing what appear to be magical acts because their former leader instructed them to do things in a certain way. Of course, from the short run perspective of NIE in which a decade is a long planning period, a bet on an institution built around a guru and his disciples at the peak of the guru's power is a good bet, one that would undoubtedly pay off.

Paul Lazarsfeld, in an interesting semi-autobiographical account of his career, makes a distinction between men who use research organizations to further their career and those who serve the institution and identify their careers with the fate of the latter. Such "institutional men" as Lazarsfeld termed them are more concerned with the success of the research organization than with their own prominence, although it must be admitted that the latter serves the former. An institutional man does not seek disciples as much as he seeks for staff members whose careers, when they skyrocket, will be viewed as closely identified with the institution in question. Assuming that Lazarsfeld has identified another form

of charismatic leadership, it would be one whose leadership is built upon organizational facilitation.

I have dwelt at length on the phenomenon of charisma because I believe that charismatic leadership is almost essential to the success of a research organization, especially success that makes such an organization stand out among the general run-of-the-mill. Especially importnat is the type of leadership that works best in group settings. A brilliant and admired leader who is essentially a solo scholar or researcher may be an ornament to a research organization, but will not make much of a contribution to the work of those about him. A leader who fosters discipleship will do better, but best of all would be one who can tolerate diversity in style and content and who sees at least some of his rewards coming from reflections of the accomplishments of others.

Hence one of the critical points in the assessment of the capacity of an institution is its proposed leadership. Ideally one would like to have a person whose accomplishments have attracted admiration and who have a track record of working with and through other persons -- usually at a level junior to that of the leader -- with track records established for those who have worked with that person.

On the negative side this means:

.....excluding persons who have produced little that is regarded as important in the field in question

.....excluding persons who have had the opportunity to work with others, including students, and have produced few cooperative products or few students identified with that person

On the positive side, it means:

.....favoring persons with records of producing work that is regarded as important in the field in question

.....favoring persons who have records of working with others and especially in the mentor role for budding reserachers

To some these recommendations may sound like a recipe for perpetuating the Establishment in a field where the Establishment may be one of the obstacles that stand in the way of progress. Young persons who have not had the opportunity to establish track records, persons who have not had the opportunities to work with students and colleagues would be systematically overlooked in an assessment if one applied these rules stringently. It must be admitted that there is some truth to such criticisms. Yet it is difficult for me to imagine that some one proposed as the leader of an institution would not have had some opportunity to work with others, establish at least some reputation within some circles, and have had some access to the training of graduate students. Obviousl a person with a decade of professional experience beyond the Ph.D. would be easier to judge than someone with only two or three years, but I doubt that there are many proposals for R&D centers that are headed by 25 year olds as principal investigators. More likely the lower bound on the ages of proposed principal investigators is 35, an age by which a track record should have been established.*

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*Persons who may have been prevented from establishing a track record -- e.g., women and minority group menters -- may be accommodated within these assessment guidelines by considering their professional lives as considerably shorter than the chronological time between their terminal degrees and the date of application.

One may raise the question why a young (or old) hotshot would want to be the principal investigator in an application for funds to set up an R&D center? The answer to this question must be sought in the internal economy of organized research and scholarly activity, a topic we will take up in the next section. But it should also be borne in mind that one of the characteristics of highly successful persons is that they tend to be risk creators as well as risk takers. To set up an organization means to take a risk, if your goal is success and not merely the attainment of a higher level of affluence in amenities and followers.

The Micro-Political Economy of Research Organizations:

The assumption upon which the funding of R&D centers is based is that it would be possible to better accomplish tasks through the funding of organizations than it would be possible to accomplish through some alternative means, the major rival being project funding. In this particular case it appears that the hope was that specific goals would be furthered more directly and more rapidly and that personnel would be attracted to work toward such goals who might otherwise have spent their time.

There are many successful models for such centers, most of which lie outside the social sciences: R&D is obviously an activity that cna be carried through successfully in some fields. However, it should be borne in mind that "hard science" R&D is rarely carried out within a university setting. The National Laboratories set up by the Atomic Energy Commission were established as separate institutions, with strong

ties to but not within universities. The R&D efforts that sprung up around MIT and Johns Hopkins (to mention just two) were carefully placed some distance from the campus and in some cases actually spun off as separate enterprises (e.g., Stanford Research Institute and Lincoln Laboratories). Others with distinguished histories (e.g., Battelle Memorial Institute) were always separate organizations. There may be some institutional wisdom exemplified in this pattern, perhaps expressing the extent to which R&D activities and old line academic departmental activities may be incompatible.

I suspect that the incompatability is especially strong in the physical sciences whose basic science and engineering activities have been more clearly separated, than in the social sciences. In the social sciences engineering disciplines have yet to appear as separate activities expressing the fact that basic research and applied research shade into each other so gradually that the separation is not yet possible. Yet, there is some antagonism between the two activities, as indicated by the perennial debates over the proper role of social scientists in social policy and of the relative importance of applied versus basic research. I mention this antagonism in this context because it should be borne in mind that an R&D center is by no means a completely attractive addition to a university campus. Some faculty members regard R&D activities as at least second class and Center personnel as occupying second or lower ranks as scholars or researchers. It should be noted, however, that university administrators are more accepting of applied research centers than academic departments.

One important element in the assessment of institutional capacity is the assessment of the organizational forms proposed for R&D centers. Elsewhere* I have suggested a general principle for the evaluation of research organizations, as follows: a research organization is worthwhile to the extent that participation in the organization enables its scientific staff to accomplish more in the way of research than would be possible outside the orgainzational context. This rule is obviously phrased in a form that is most useful to an individual contemplating joining an organization or a university in fostering its founding. For an outside agency interested in sponsoring such organizations, the reference frame would have to be shifted only slightly since a main alternative to R&D centers would be the funding of individual projects and hence individual researchers or ad hoc groups of researchers. Hence if a proposed R&D center could provide something to both NIE and the social scientists that constituted the center that would be over and above what each could accomplish in the absence of the center, then such a center would be worthwhile funding. Obviously crucial questions arise at this point:

.....Can there be a strong interest in the substantive goals of NIE shared by members of an R&D center?

.....Just how much additional productivity allocatable to an R&D Center would make it worthwhile?

The first question addresses itself to issues of committment to

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*"Observations on the Organization of Social Research" in Richard O'Toole (Editor) The Organization, Management and TActics of Social Research. Cambridge, Schenkman, 19/1.

substantive goals and the internal organization of research centers while the second is concerned with the prediction of levels of productivity. The second question is more difficult to answer, and consideration of the issues involved will be left to the end of this section.

Social research institutes that are more than letterheads and a few secretaries tend to follow either the model of a "consortium" or of a "research firm."* A consortium is a relatively loose confederation of faculty members held together primarily by their interest in access to the resources held by the collective entity involved and who have developed a very low level of division of labor among themselves. Most existing university social research centers are of this sort. Such research centers have little organizational capability to pursue a unified line of research. This is not to say that a consortium cannot produce good research, butmainly that it is not likely that a consortium will pursue a single line of research or a relatively small number of goals. A consortium is a coalition of several very independent researchers, each of whom guards his autonomy quite carefully. Indeed, a consortium tends to mirror academic departments in their structure, especially with regard to mounting an integrated effort to accomplish some one or small set of goals.

A research firm, in contrast, is one in which lines of authority are clearly drawn and in which a director or directorate clearly has the

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*These distinctions, as well as some less well developed forms of social research organizations, have been discussed in greater detail in a previous publication: ibid.

right to provide direction to firm members and to oversee both the processes or producing research and the quality of the end product. The division of labor in a research firm tends to be relatively elaborate. It is difficult to imagine that an academic department in the social sciences would easily form a research firm, although it is very easy to imagine the development of a consortium. The main reason for this prediction lies in the small likelihood that department members of relatively equal status and considerable individual autonomy will enter into arrangements in which some one of their members will have considerable authority over their activities and thus their individual autonomies would be lessened.

There are circumstances under which research firms may be formed: first of all, a senior member of a department might attempt to organize a firm in which members would be mainly persons considerably junior to him. Younger members would be tempted to join -- thereby losing some autonomy -- because the resources available to them through a research firm would be appreciably greater than otherwise available than otherwise available to them (and hence membership be more attractive than to older faculty members). Secondly, a research firm could be formed within a department in which the firm did most of its staff recruitment from without the department.*

Of course, any particular research center is likely to be some combination of consortium and firm, as for example was the Johns Hopkins

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*A circumstance that would likely increase conflict between department and center since the "employees" of the firm would most certainly be regarded as inferior in quality to the "faculty" of the department.

R&D Center (Center for the Study of Social Organization of Schools) at the time I was affiliated with it. At the Hopkins Center, some of the faculty members in pertinent disciplines joined as staff members, but the R&D Center also hired persons as staff members who were not faculty although otherwise academically qualified.

It seems highly unlikely that a consortium would be able to put together an integrated research enterprise. Rather the more likely arrangement would be a collection of loosely related research projects consisting mainly of long standing interests of the faculty members involved re-labelled to appear as if they were relevant to the mission of the center. Thus the Hopkins R&D Center contained projects that ranged from the development of simulation games as teaching devices to the study of the effects of expectation states on learning in laboratory and controlled class room relations. The relatively junior faculty who served as Directors had little room to exert leadership leverage over prestigeful senior faculty members who were senior staff members in the Center but were colleagues of considerably greater power in the departmental power arena.

Of course, the observations from which these rather sweeping generalizations were drawn were made during a period of great growth in American universities. The relative scarcity of talent and considerable competition among universities for this scarce good enhanced the autonomy of individual faculty members considerably, and correspondingly undermined the authority position of department chairpersons and even academic deans. Times have indeed changed in the middle seventies, a shift in fortunes that

appears to be with us for many years to come. Faculty mobility has been appreciably lessened as the market demand for faculty has weakened. Universities one by one have come face to face with the necessity to cut back on budgets and consequently on support services and eventually staff. Research funding has become hard to get. All these trends point to a lessening in the autonomy of faculty members and a consequent strengthening of the power and authority of department chairpersons, deans and other administrative officials.

As usual, the greatest loss of power will be felt by those who have least to begin with. The position of the Ph.D. newly launched on his career is hardly enviable, but neither is the position of the untenured instructor or assistant professor. The attractiveness of non-department research positions will be thereby increased and the willingness of faculty to join in research centers (especially when they are well financed) can also be expected to increase.

These new trends in academia make it more likely that research firm types of research centers can be formed both within university frameworks or as attachments to universities. How strong these trends are and whether they have reached their maxima is not at all clear.

To return to the second question we raised at the beginning of this section -- how much additional productivity justified the setting up of a research center? It seems to me that there are two measures of "additional" productivity that need to be applied in the case of NIE's search for appropriate R&D model types. First, there is a productivity represented by an

appreciable shift in research output. If the research establishment going its own way would produce X amount of relevant research, then an increment to that X amount of relevant research is worthwhile. Secondly, there is increased productivity in the sense that the research establishment would produce more of what it was going to produce anyhow (X + Y....+Z) but not necessarily any more of X than of any other output. Obviously, the most desirable outcome would be more of X and an increment over X than would be the case were the same shift to take place without the establishment of research centers. This would seem to be the best justification for R&D Centers as opposed to project funding. One could imagine a wisely administered project grant program that would shift research priorities in the field but that the newly established priorities would be pursued at the same level of productivity as before.

It seems to me that the combined goal of priority shift and increased productivity within the new mix would be best accomplished by establishing research firms that would bring into their orbits persons of considerable talent who would have been working on other problems. The re-packaging of existing research personnel already working on R&D efforts into research centers might increase their productivity but is not likely to shift priorities markedly within the research establishment as a whole.

The main considerations laid out in this section point in the following directions for assessments of institutional capability. First, research organizations that have strong authority lines -- i.e., that are laid out more like research firms than as consortia -- are more likely to be

productive and more likely to stick to a limited number of missions. Secondly, proposed research centers that would incorporate into their staffs persons who would be shifting their research emphases are most likely to produce an appreciable increment in the total R&D output.

The Issue of Intra versus Inter-Disciplinary Centers:

It is clear that education is not one of the basic social science disciplines. Its theoretical structures have been built on the bases of extensive borrowings and modifications of borrowings. It practices are a collection of craftlore knowledge built up on the basis of considerable experiences but not yet completely integrated with the body of eclectic educational theory. Indeed, in this respect education comes closer to being an engineering branch of the social sciences than almost any other social science activity. Of all the social sciences, psychology has been the greatest donor of theory and suggested practice to education and should probably continue to be in the future. Sociology has provided some things to education, more so than either economics or political science.

The miscellaneous background of education argues, at least on the surface, for R&d to be an inter-disciplinary activity. After all, it is obvious that the learning process is the domain of the psychologists, that the design of organizations is the province of sociologists, that the political nature of education makes that institution a matter of concern for political scientists, and the fact that the educational enterprise produces a form of human capital makes it a matter of concern for economists.*

*There is no intrinsic reason to stop with th ese disciplines since there is good reason to add the life sciences, branches of medicine, not to mention history, communications, anthropology, environmental design, etc.

Yet the history of inter-disciplinary activities has not been a happy one. Inter-disciplinary departments have eventually succumbed to strong centripetal forces, e.g., the social relations departments at Harvard and Johns Hopkins and inter-disciplinary research centers have survived mainly by developing separate departments within them representing the major disciplines involved (e.g., the RAND Corporation or the Urban Institute), usually with one or another discipline emerging as the dominant species. Indeed, the history of education as an academic field is one in which the basic disciplines have tended to develop their own subdepartments, as for example, educational psychology and sociology.

There are obviously strong forces that make inter-disciplinary activity involving the close cooperation among disciplines difficult in the social sciences. Partly, it is because the disicplines exert such strong career pressures on their members. For a sociologist to be an important researcher in the field of education does not mean very much unless there are other sociologists also working in that field. It is also the case that the disciplines compete with each other intellectually. I have watched sociologists and psychologists within an R&D center plan researches to show that the other discipline's researchers were wrong about their understanding of some phenomenon.

The best interdisciplinary work arises not so much out of the cooperation of persons across disciplinary lines but out of the incorporation within one discipline of the theory and knowledge developed in another. Thus the new field of biophysics arose mainly because some physicists taught themselves biology and some biologists taught themselves physics.

And it solidified as a discipline when both adopted the same theoretical and research paradigms. Similarly the great influence that econometric models is having on sociology today arises from the fact that some sociologists have taught themselves econometrics rather than out of the cooperation across disciplines of economists and sociologists.

These considerations argue against trying to establish interdisciplinary R&D centers. Rather it would be better to fund centers in which the senior personnel were ecumenical in their training and interests.

Buildings, Monuments, Facilities and Amenities:

Nothing seems more obvious at first glance than the influence of micro-enrivonments on intellectual productivity. A fine building, commodious quarters, reasonable working temperature and sound levels all seem to be elements that should facilitate individual productivity. Similarly it would also be logical that electric typewriters, sophisticated terminals connected to the best of scientific computers plus laboratory equipment of the best and latest design should also make life a lot easier.

There is no reason in the world why researchers and research organizations should not have the best equipment and quarters available to them. However, the basis for this judgement is more humaneness than on firm evidence supporting the influence of environments and facilities on productivity. The variety of places, the range of amenities available in which organizations of different types have been located suggest that such facilities are not necessary for productivity but are nice to have anyhow.

Perhaps the only firm environmental requirement that a research center should have is sufficient space to house all of its scientific personnel

in close proximity to each other. Research organizations that have had to separate into different buildings -- even though they might be just a few yards away -- have suffered from a developing sense of intra-organizational divisiveness, a disease that often restricts the kind of easy access of colleague to colleague that is one of the more important benefits of organized research.

Lions and Foxes: The Problems of Succession:

Everyone agrees that R&D is a process that extends over a number of years and can hardly be accomplished instantly. The extended character of the effort necessary makes the problem of succession a critical one. How can one achieve continuity of effort when one can anticipate some turnover, possibly large, in personnel over the period in question?

One of the more attractive features of research centers as opposed to project funding is the expectation that centers would provide a greater continuity of effort. The organization accepts responsibility for its tasks, thereby accepting also the responsibilities to seek out and recruit replacements for losses as well as the supervision of effort to assure that proper levels are expended. These are functions that are the responsibility of a center director or directorate to exercise, an organizational fact that further accentuates the importance of leadership. I would estimate that a very significant proportion of center directors' time would be expended in the problems brought about by the need for constant coddling, cajoling of existing personnel and the recruitment of replacements for those who have left.

Hence the critical points in a center's history comes when the problem of succession strikes at the director's office. If a director has been notably successful in his role, the problem seems especially serious. But, there are several things that ought to be borne in mind in thinking about the problems occasioned by a succession crisis:

First, success makes succession more of a problem. A person who has demonstrated success in building a research organization increases his market value by that accomplishment. Other institutions would like to have an institution builder and hence a successful director is likely to have many temptations set before him. One of the ways of countering this possibility is to provide super-grade amenities for a director: he should be paid considerably more than his colleagues of comparable rank and the institution sponsoring the center should be prepared to meet offers that would be made to a successful director.

Secondly, problems of succession often provide important opportunities for a change in the management of an R&D Center. A director who is superb at the task of getting a center under way may not be the best person for a long haul effort. The quick, nervous forays of a fox may need to be succeeded by the slower majestic stance of a lion. An institution that has done very well under the initial impetus given by a very exciting, intellectual charismatic leader may need to be followed by someone whose main task it would be to see that the thing started by his predecessor are carried out. Indeed, one might want to institutionalize the idea of a alternation of lions and foxes in the leadership of research organizations (as well as universities and business enterprises).

Burying the Dead:

Organizations tend to persist, finding some excuse and rationale for existence no matter how tenuous the connection. Indeed, part of the problem of the existing R&D center setup, if I read the Roald Campbell et. al report correctly is the considerable political pressure created by the trade union of existing R&D centers and Regional Labs pressing for further support. From what I can gather in the Campbell et.al. report, the existing centers with few exceptions have produced no results or disappointing results. Of course, there are a variety of reasons for the wholesale failure, some of which may justify the claims of the trade union for continued support of the centers and labs. Whatever the justice or injustice of the present situation, a major problem is exemplified by the existing centers: How can an organization be buried when it is morally and intellectually dead but the body is still twitching?

It doesn't take much astute thinking to realize that this problem is one which plagues all political and quasi-political organizations. It is as, or more difficult to close an army base, renegotiate a union contract, turn off contracts to a contractor, and so on. The problem stems from the fact that any moderate sized installation can form a constituency to which elected officials may be willing to pay attention: R&D Centers and Labs are big enough in some places to form such constituencies and, with proper guidance, make considerable impact on congressmen and senators, especially in circumstances in which NIE is already perceived as an agency with some gross political deficiencies.

Regardless of the source of the problem, it still remains: Organizations resist dissolution. This suggests that one's mistakes linger longer than one desires and hence that false positives are more costly than hinted at in the first section of this paper.

There are a host of legal devices to insure that legal obligations to support centers are not unwittingly incurred. Such procedures are familiar enough to federal contract writers, that little need be said on this score. Indeed, there is little else that can be added. It is a problem: there is no easy way to cut off an obligation even when it is only a "moral" one. About the best that can be said is that it is easier to do such radical surgery with a clear conscience when the object of the amputation has not been misled into expecting much more in the way of long term support. In short, negative evaluations ought to be given as soon as they appear to be even slightly justified in order not to produce the experience of injustice through surprise.*

Assessment of Institutional Capacity?

The preceding sections of this paper have wandered through thickets of considerations, warnings and a few alarums and excursions. The main impression the pages should leave with the reader are as follows:

First of all, there is no set formula, no checklist of items, no linear equation, that will provide an accurate assessment of institutional

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* Indeed, the same tactics that are designed to produce failure are the same ones which make it difficult to extricate from a bad contract Feelings of injustice are more prone to arise in circumstances where feedback messages have been unclear and where the goals set have been vague and subject to managerially induced fluctuations.

capacity for R&D. I have tried to make some generalizations, but it is clear that there are enough exceptions to wonder whether the correlations are of the order of .2 or .7. The possible range of magnitude of predictive efficiency is very great.

Secondly, I have put forth the view early in the paper that false negatives were more serious than false positives and hence that the selection criteria should be lenient to the point of flexibility, consistently favoring applicants or existing centers when there is some chance of success.

Thirdly, I have placed a great deal of emphasis on the leadership cadre of a research center as the major consideration. I propose that the best bet for a center to be creative is to have a young (35-45) scientist as its director who has a good track record as far as his own contributio s are concerned and has a good track record either in working with others and working with his juniors.

Thirdly, I recommend that the consortium form of research organization is to be rejected except under rare circumstances. A "research firm" is necessary in order to have a concerted attack on a narrow goal, as R&D should be defined.

Finally, I point out that history has produced a period that will be increasingly favorable to organized research. The tight labor market will make organized research more attractive to young people. For older scholars, the scarcity of research funds for projects makes applied research within an organized center more attractive.